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Author's response

Dr. Payne-James

Re: Ebrahim IO. Somnambulistic Sexual Behaviour (Sex somnia). Journal of Forensic and Legal Medicine (Formerly Journal of Clinical Forensic Medicine) 2006;13:219–224

Thank you for forwarding to me a letter from Dr Pressman and Colleagues (Pressman & Co.) regarding the article listed above. I have reviewed the letter and as requested, I am submitting my response below.

Background

As you mention in your email, the article was published in a special edition of the *Journal of Clinical Forensic Medicine* (JCFM) earlier this year. The article is a Case Report and was subject to the journal's limits on length and number of references. As such, it was not the primary intention of the article to provide a detailed review of the area concerned, rather to provide “one or two related cases with specific unambiguous message that needs little discussion, small number of references” (this statement is taken from The Guidelines to Authors from the JCFM). In addition, the case report underwent peer review and was subject to revisions prior to publication as is standard practice for scientific journals. In assessing my response to the letter by Pressman & Co, I have re-examined my original and revised manuscripts, the reviewers' comments, my final submitted manuscript and the published article.

I will now deal with the specific comments by Pressman & Co:

Statement 1

Whilst I agree that the initial statement is an older definition, the paragraph continues and states clearly:

The most commonly implicated sleep disorders associated with automatism are the Parasomnias of which the Disorders of Arousal are the most common. These include sleepwalking disorder.

There is thus no basis for Pressman's statement that “... It was not stated that it was a disorder of arousal.” It was stated in the article that sleepwalking was a sleep transition disorder. It is interesting how old science becomes new science. A transition disorder suggests that sleepwalking always arises from an arousal, with the implication that the arousal is external. Current views of sleep physiology suggest that we do not yet understand the arousal mechanism, and it is far too early to suggest that all arousals for sleepwalking are external. For example, there is an excellent review by Colrain (Colrain IM. The K-complex: a 7-decade history. *Sleep* 2005;28:255–73.).

Statement 2

The article clearly and unambiguously states that “**Rarely, it can appear for the first time in ADULTHOOD** (my capitalisation)

The authors of the letter have clearly not read the paper properly as they have misread this sentence in which they confuse childhood with adulthood. Since the authors then elaborate on this reading error it raises the question of whether all the signatories actually read the article before signing the letter. One would have expected at least one of them to have picked up such an elementary error in their letter. In the same paragraph the writers note that they are aware of only seven cases in total where sleepwalking occurs in association with pyrexial illnesses. It is a

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commonly reported finding that children who sleepwalk tend to have sleepwalking episodes precipitated by pyrexial childhood illnesses. I am not sure why the authors refer to only seven cases. The authors also make a distinction between physical illnesses with and without fever. This is clearly an inaccurate distinction as an illness with fever is a physical illness. The authors state that the current estimates of sleepwalking in adults are 4% whereas a more conservative figure of 1–2% was given in the article. This figure does of course vary with the studies quoted and is not yet absolute as populations differ.

Statement 3

The question of confusion after a sleepwalking arousal is vitally important. The question of how long the confusional episode lasts is also important. It is certainly true that patients awoken from sleep with high blood alcohol levels will show confusional behaviour on awakening. Some of this could be due to a sleep disorder and some will be due to the fact that they are drunk. This is in no way a contentious statement and simply states that if prolonged periods of confusion are noticed, then high blood alcohol levels could be a causative factor. There is no statement in this section of the article that claims that this is a diagnostic criterion and the preamble states unambiguously: *“the following proposed guidelines have been compiled from the medical literature.”* – the operative words being *proposed* and *guidelines*.

Statement 4

At no point do I state that these are diagnostic factors. The references from which this list has been compiled are given in the three major references quoted. It is impossible to do a prospective study on serious violence and sleepwalking. Thus the data comes from retrospective accounts of violence as reported either in forensic or clinical settings. It is important that these be included in a list of possible factors which are being considered by the sleep community as being significant in cases of sleep violence. The readers of the article have their attention drawn to the three main references and they will be then able to assess the significance and quality of the data. It would be wrong to omit these from the list on the grounds that they have not been tested in a sleep laboratory.

Statement 5

As noted above, this statement follows a misreading of the article. The *example* of head trauma was used for illustrative purposes only. Other causes include medication, etc. as mentioned in the article. I did not see a need for repetition considering that this was a Case Report!

Statement 6

Having reviewed my manuscripts it is clear that during the course of the process of submission, peer review, revisions, etc., a single word **SCORING** (meaning analysing the sleep study) was removed/left out.

The sentence the authors refer to should have read:

“Internationally accepted criteria are used for **scoring** these studies” – read in this context the reference to Rechtschaffen and Kales Sleep Scoring Manual is appropriate and correctly placed.

The sentence then continues to describe the three night protocol used in our laboratory which is a matter of fact statement with no implication as to its international acceptability.

I note that Pressman & Co have mistakenly numbered this as reference #19 in their letter when it is actually reference #12.

Furthermore, I note the comment from Pressman that *“in the forensic cases in which this has been employed by the author, no complex behaviours have been provoked”*

This statement is incorrect – Mr Pressman cannot know of all the cases we have assessed and cannot therefore justify this statement.

Statement 7

With regard to the EEG features that may or may not be associated with a tendency to sleepwalking, I have cited the correct references that argue for and against the sensitivity, specificity and reliability of these measures. It was not the intention of this paper, a case report, to provide a thorough review of the literature in this area – that is a separate type of scientific paper called a Review Article. The reader is provided with the references and it is left up to the reader to explore in depth the arguments and science surrounding this area of knowledge gathering. Nowhere in this article have I said that these features are of themselves diagnostic nor stated that they were sensitive or specific for the diagnosis. Quite the contrary, I have clearly stated that controversy exists.

The statement in the article “Despite this controversy useful indicator of electrophysiological instability in stage 4 sleep” does not mention sleepwalking merely a state of electrophysiological instability. I note and accept the disagreement from the authors and will address this further on my list of suggested corrections later.

Statement 8: regarding provocative tests

Firstly, the article does not state that these are diagnostic tests – these techniques have been used in an attempt to provoke or stimulate a sleepwalking episode or a related complex behaviour – sleep interruption and sleep deprivation have been used in sleep labs for many years and are thus popular techniques. The use of bedtime dose of alcohol as part of the assessment of forensic

sleepwalking cases was introduced by our team in the UK to assess these individuals in whom alcohol was thought to be a trigger factor.

Secondly, the role of alcohol as a trigger, priming or facilitating factor in sleepwalking is well established. It is essential, particularly in the forensic setting to be thorough and assessing that particular individual's response to alcohol is important – the primary reason for using the Alcohol Provocation is to assess the ability to trigger sleepwalking or related complex behaviour.

Thirdly, alcohol is a CNS depressant and is known to lower the seizure threshold and thus may also provoke other neurological disorders such as Epilepsy which may present as nocturnal wandering or violence.

Finally, alcohol has been used in many forensic psychiatric investigations for a large number of years. One of the questions that needs to be answered if a patient has a confusional episode at night after alcohol has been taken, is whether the brain response to alcohol is normal or abnormal. There is a large literature looking at episodic dyscontrol where alcohol has been involved in a crime and where alcohol is used in a clinical setting to see if it is possible to precipitate the behaviour due to an abnormal brain response to alcohol. This is widely known and commonly used. Combining alcohol with the polysomnogram is important as not only is information obtained about the interaction of cerebral function and alcohol but also about the possibility of alcohol leading to a sleepwalking episode.

Our group has yet to publish our findings from a series of evaluations involving a bedtime dose of alcohol in sleepwalkers. We will no doubt invite comments from our learned colleagues prior to publication.

Statement 9: regarding alcohol and sleep

It is correct that most studies support a dose dependent suppression of dreaming sleep and increased deep sleep. This is not a contentious statement. In this case, the defendant was not a binge drinker, alcoholic or former alcoholic. The information provided here is thus accurate, appropriate and scientifically valid.

Statement 10: regarding the reference #17

I would like to thank, the authors for pointing out the incorrect placement of this reference. The reference should correctly and appropriately be placed in Section 6.2 where sleep deprivation is discussed.

Statement 11: regarding the features implying a tendency to sleepwalk

I agree with the authors that this statement does not reflect current state of art in sleep medicine. This was pointed out by the reviewer of the article prior to publication and, to my memory, the paragraph was substantially changed to reflect the relationship between sleep disordered

breathing sleepwalking and the EEG. Unfortunately, the final version is not that which was published.

This was an oversight for which I unreservedly apologise.

I suggest the alternative wording below:

He showed significant findings on the PSG including HSDWA, DWC, a fragmented first slow wave sleep cycle (SWS) with multiple HSDWA arousals and lower SWS in the first sleep cycle. Whilst these findings are found more frequently in sleepwalkers they are also found in patients with sleep apnoea and in normal controls. They are in no way diagnostic of sleepwalking and remain of interest for future research.

I trust that the authors of the letter will be able to empathise with how easy it is to make an oversight in reading and writing.

Concluding remarks

I am pleased that there is unanimity with regard to the diagnosis of Sexsomnia in this case report.

I suggest that the article remain with the following corrections published:

1. Page 222, 1st Column, Line 29: The word *scoring* be placed in the sentence beginning "International Criteria are utilised for *scoring* these studies. . .".
2. Page 222, 2nd Column, Line 7–10: Should be changed to: Despite this controversy, *some* sleep experts agree that these EEG events remain a useful indicator of electrophysiological instability in stage 4 sleep".
3. Same paragraph – the reference 14 should be removed.
4. Page 222, 2nd Column Line 6 from bottom: Reference #17 should be moved to Line 25 from the top at the end of the sentence following references 9 & 10.
5. Page 223, 1st Column, line 19 from bottom to line 11 from bottom should be replaced by this paragraph:

He showed significant findings on the PSG including HSDWA, DWC, a fragmented first slow wave sleep cycle (SWS) with multiple HSDWA arousals and lower SWS in the first sleep cycle. Whilst these findings are found more frequently in sleepwalkers they are also found in patients with sleep apnoea and in normal controls. They are in no way diagnostic of sleepwalking and remain of interest for future research.

I trust that we are able to move forward in a professional manner. I thank the authors for their comments and look forward to hearing back from your learned self.

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